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**Social Interaction Effects in  
Disability Pension Participation**  
Evidence from Plant Downsizing

**Abstract:**

We estimate the magnitude of social interaction effects in disability pension participation among older workers in Norway. Specifically, we investigate how a worker's propensity to draw disability benefits is affected by a plausibly exogenous shock to the disability entry rate of similarly-aged workers in his or her neighborhood. The problem of omitted variable bias is addressed by employing a novel instrumental variable (IV) strategy, using plant downsizing at neighbors' plants of employment as an instrument for the disability entry rate among one's previously employed neighbors. Our IV estimates suggest that a one percentage point increase in the participation rate of previously employed neighbors increased the subsequent 4-year entry rate of workers by about one-half a percentage point. Numerous robustness and specification tests appear to support the validity of the identifying assumption in our IV strategy.

**Keywords:** disability, downsizing, layoffs, plant closings, social insurance, social interaction, welfare norms

**JEL classification:** H55, I12, I38, J63, J65

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# 1. Introduction

Understanding the determinants of disability participation is becoming an increasingly important issue for policy makers. Between 1980 and 1999, the share of non-elderly adults receiving disability benefits in the United States increased 60 percent to 4.7 percent.<sup>1</sup> Across the OECD as a whole, disability participation rates increased 36 percent over the period, to 6.4 percent.<sup>2</sup> The dramatic growth in disability participation rates has important implications for national productivity and the public financing of disability benefit programs. In 1999, disability benefit payments comprised 1.4 percent of GDP in the U.S. and 2.5 percent of GDP across countries in the European Union.

Notably, the substantial growth in disability rolls has occurred without any change in the prevalence of self-reported disabilities (e.g. Burkhauser et al. 2001; Cutler and Richardson 1997; Duggan and Imberman 2006). In contrast, convincing evidence exists that economic conditions affect disability participation. Black, Daniel and Sanders (2002) demonstrate that the coal boom and subsequent bust had a large impact on disability participation in U.S. coal-producing states. Autor and Duggan (2003) find that decreasing demand for low-skilled workers and increases in their disability benefit replacement rate have led to large increases in the disability participation of high school dropouts. Autor and Duggan (2006) also cite the increasing real value of Medicaid benefits and liberalization of the screening process as contributing to increased rates of disability insurance use in the U.S. These and other studies suggest an important role for non-health factors in workers' decision to apply for and draw disability benefits.<sup>3</sup>

In this paper we empirically investigate the magnitude of social interaction effects in disability pension (DP) participation in Norway. Specifically, we investigate how a worker's propensity to draw DP is affected by a plausibly exogenous shock to the disability entry rate of similarly-aged workers in his neighborhood. A large and growing empirical literature suggests an important role for social interactions in many behavioral outcomes including teenage childbearing (Crane 1991), educational attainment (Sacerdote 2001; Hoxby 2000; Lalive and Cattaneo 2005), saving decisions (Duflo and Saez 2003), criminal activity (Case and Katz 1991; Glaeser, Sacerdote and Scheinkman 1996; Katz, Kling and Liebman 2001; Ludwig, Duncan and Hirschfield 2001; Kling, Ludwig and Katz 2005) and welfare participation among ethnic minorities (Bertrand, Luttmer and Mullainathan 2000; Aizer and Currie 2004). If social interaction effects exist in the context of disability insurance, it could help

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<sup>1</sup> Statistics on disability program use and expenditures obtained from OECD (2003).

<sup>2</sup> Throughout this paper, we employ the colloquial expressions "on disability" and "disability participation" to refer to the utilization of disability pension benefits.

<sup>3</sup> See Rupp and Stapleton (1995) and Stapleton et al. (1998) for related studies on the impact of economic climate on the application and receipt of disability benefits.

explain the wide variation in disability participation across geographic areas (McCoy et al. 1994) and over time. Moreover, the magnitude of such effects is critical for predicting the impact of policy reforms, demographic changes and economic shocks on disability participation rates.

In the context of disability participation, social interaction effects could potentially operate through a number of mechanisms. For example, social norms against disability participation could reduce the desirability of participating by imposing a utility cost in the form of social stigma (Moffitt 1983; Lindbeck, Nyberg and Weibull 1999).<sup>4</sup> The magnitude of this stigma is expected to decline as disability participation among one's peers increases, thereby reducing one's utility cost of entering disability. In this way social interaction effects give rise to a social multiplier that amplifies the effect of policy changes and economic shocks on aggregate participation rates (see e.g. Brock and Durlauf 2001; Glaeser and Scheinkman 2003). Any change that *directly* affects individuals' rate of disability use will have an additional *indirect* effect through the influence that one's participation has on others. Identifying social interaction effects in observational data is complicated by problems of omitted variable bias.<sup>5</sup> Peers are likely similar in ways unobservable in data and are also likely subject to similar unobserved shocks. In this paper, the problem of omitted variable bias is addressed by employing a novel instrumental variable (IV) strategy similar to the "partial population intervention" approach advocated by Moffitt (2001). Our strategy hinges on the empirical observation that plant downsizing events have a substantial effect on disability entry rates (Rege, Telle and Votruba 2007). We therefore use plant downsizing at neighbors' plants of employment as an instrument for the disability participation rate among one's previously employed neighbors.<sup>6</sup> The intuition behind this approach is straightforward: if social interaction effects exist, then neighborhoods disproportionately affected by plant downsizing events should exhibit a relative increase in disability entry rates, even among workers not directly affected by downsizing.

Social interaction effects estimated under this IV strategy will not suffer from omitted variable bias provided downsizing rates in neighbors' plants of employment are uncorrelated with unobservable determinants of DP participation. This identifying assumption is potentially problematic because downsizing events concentrated within a particular neighborhood could reflect or cause a decline in local economic opportunities. Alternatively, plant downsizing may be concentrated in neighborhoods populated by persons with higher propensities to utilize DP. The richness of our data,

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<sup>4</sup> Social norms are only one possible channel through which social interaction effects might operate in disability participation. Section 2 discusses two other possibilities: leisure complementarities and information exchanges.

<sup>5</sup> Manski (1993, 1995) catalogs the range of estimation problems in observational studies of social interaction effects, though our terminology varies somewhat from his. In particular, what we label "social interaction effects," Manski refers to as "endogenous effects."

<sup>6</sup> Throughout, we use the term "plant" to refer to the establishment at which a worker is employed, which is distinct from the firm of employment (as firms can consist of multiple plants).

an 11-year panel dataset containing socio-economic information, employment data, and disability participation records for every person in Norway, allows us to indirectly test the validity of our identifying assumption.

Our analysis indicates that social interaction effects play an important role in DP participation. Our IV estimates suggest that a one percentage point increase in the participation rate of previously employed neighbors increased the subsequent 4-year entry rate of workers employed at the end of 1999 by roughly 0.5 percentage points. This has important policy implications, suggesting the direct effect of demographic shifts, policy changes, health shocks and economic shocks on disability participation understates (by roughly one third) the full response expected in equilibrium.

## 2. Social Interaction Effects

The logic of social interaction effects rests on notions of utility interdependence. That is, when one's peers engage in a particular behavior, it can potentially affect one's own utility from engaging in that behavior. In the context of disability participation, this interdependence could operate through at least three channels: social norms, information and leisure complementarities.

Disability participation is likely affected by social norms regarding "appropriate" participation behavior.<sup>7</sup> Coleman (1990) defines a social norm as a rule of behavior that is enforced by social sanctions, which can take the form of stigma. Social interaction effects arise if social norms are conditional in nature, that is, when the stigma associated with not adhering to a norm is felt more strongly when one's peers adhere to the norm. For instance, a person with a marginal disability would likely feel a higher degree of social stigma from drawing disability benefits if surrounded by peers devoted to their work. Thus, as disability participation increases among one's peers, the incentive to apply for DP among non-recipients is expected to increase.

There exists some empirical evidence that suggests an important role of social norms in welfare utilization. Though not specific to disability programs, Moffitt (1983) finds evidence for a stigma related disutility of welfare participation. Horan and Austin (1974) document negative self-characterization and lack of self respect among welfare recipients. Flaa and Pedersen (1999) document that 20 percent of welfare program recipients in Norway feel a loss of social approval.

In addition to the stigma associated with social norms against drawing disability, navigating the application process may incur a cost in terms of time and frustration. Peers familiar with this process can be a valuable source of information for would-be applicants, reducing the cost of filing an

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<sup>7</sup> See Moffitt (1983), Besley and Coate (1992) and Lindbeck, Nyberg and Weibull (1999) for theoretical models of social norms and economic incentives in the welfare state.

application. This information transfer implies that the cost of applying for disability is lower when more of one's peers draw disability.

Alternatively, a person on disability will have more time available for leisure activities than one engaged in work. Disability participation by one's peers can increase one's value of leisure, making it more attractive to draw disability. Similar to social norms and the information channel, this implies that a person's likelihood of drawing disability increases when participation among his peers increases.

Regardless of the channel through which social interaction effects operate, these effects give rise to a *social multiplier*, and possibly to multiple equilibria, that amplifies the effect of policy changes, demographic shifts and health or economic shocks on aggregate participation rates.<sup>8</sup> Any change that directly affects an individual's likelihood of drawing disability will have an additional indirect effect through the influence that the individual's participation has on others. For example, if an economic shock decreases the opportunity cost of drawing disability for a subset of workers, the subsequent increase in disability participation could reduce the stigma associated with drawing disability, thereby increasing participation rates even among those not directly affected by the shock. This self-reinforcing process continues until a new equilibrium is reached.

### 3. Disability Pension Program in Norway

The Norwegian Disability Pension (DP) program<sup>9</sup> serves a similar function as the combined disability programs of Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI) in the U.S. A basic and a supplementary pension provide a benefit that is increasing and concave in prior earnings similar to SSDI, and a special supplement ensures a minimum benefit amount similar to SSI. Even though the Norwegian and U.S. programs have similar benefits formulas, increasing at a decreasing rate in past earnings, the Norwegian disability program is more generous, providing a higher earnings replacement rate particularly for low income workers.

Another important difference between the Norwegian and U.S. programs is that the Norwegian program allows workers to apply for DP while still employed. As a result, it is common for Norwegian workers to receive "sick money" prior to transitioning from employment onto disability without ever being unemployed. Sick money refers to temporary assistance (up to one year) provided to disabled workers, ensuring benefits equal to 100 percent of earnings up to some maximum level. After one year, workers can draw a somewhat smaller rehabilitation pension until returning to work or

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<sup>8</sup> For a formal analysis see e.g. Glaeser and Scheinkman (2003) or Brock and Durlauf (2001).

<sup>9</sup> See Rege, Telle and Votruba (2007) for a more detailed description of Norway's disability pension program.

entering DP. During the first 12 months of sick absenteeism, when the worker is typically covered by sick money, Norwegian law makes it particularly difficult to formally dismiss sick workers. Therefore, unlike the U.S., it is not uncommon for disability entrants to enter directly from employment. Moreover, sick money use at a given time is a strong predictor of future entry onto DP.

It is also worth noting that workers applying for disability benefits can receive a temporary disability pension if it is apparent that the worker will qualify for the permanent benefit. In measuring DP participation we include both temporary and permanent DP recipients, as the vast majority of temporary DP recipients go on to receive permanent DP.

## 4. Empirical Strategy

The current section describes our strategy for estimating the impact of DP participation among a worker's peers on that worker's decision to enter DP. Figure 1 provides a visual depiction of the timeline underlying our analysis. For the purpose of this paper, we analyze DP entry rates through 2003 of Norwegian workers age 45-63 in 1999.<sup>10</sup> For reasons that will become clear, we restrict our attention to workers who were full or part time employed in both 1995 and 1999. Therefore, our results are specific to older Norwegian workers with reasonably strong ties to the workforce.

Our hypothesis is that a worker's decision to enter DP is influenced by the DP participation of her or his peers. Defining "peer groups" from existing data sources is always somewhat arbitrary. Ideally, we would like to identify the individuals with whom a given worker interacts. Lacking such data, peer groups are commonly defined by geographic proximity and/or by characteristics suggestive of "social proximity" (e.g. similar socio-economic or employment characteristics). In this paper, we define peer groups according to proximity in geography, age and employment status. Specifically, the peer group of each worker is assumed to consist of similarly-aged native Norwegians residing in the worker's neighborhood in 1995 and, like the worker, employed full or part time in 1995.

Norway is divided into 14,211 geographically-defined neighborhoods (*grunnkrets*). These neighborhoods are small in both geographic area and population. On average, an individual lives in a neighborhood with 614 native citizens.<sup>11</sup> Thus, it is reasonable to assume that people within a neighborhood do in fact interact with each other. Peer groups are further limited to other workers in the neighborhood for two reasons. First, we might expect that workers would be more influenced by the behavior of other workers in their neighborhood than by non-workers. Thus, if social interactions influence DP participation, we would expect this influence to be observable across similarly-aged

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<sup>10</sup> We always refer to the status at the end of a year, i.e. at 12/31/yyyy.

<sup>11</sup> Among workers in our sample the mean neighborhood native population size is 691 (see Table 1 Panel C). The difference reflects our selection criteria which led to the exclusion of workers in the smallest neighborhoods.

workers residing in the same neighborhood. Second, as we describe below, our empirical strategy relies on an instrument that is specifically applicable to the DP entry of workers.

In the following sections, we describe a conventional approach to estimating social interactions effects in order to highlight the biases likely to plague such an approach. Following this, we describe the instrumental variable approach taken in this paper.

#### 4.1. Baseline Approach

By way of example, we define the following linear probability model for the likelihood that a worker who is employed in 1999 draws DP by 2003:

$$(1) \quad DP2003_i = \alpha_0 + \alpha_1 X_i + \alpha_2 N_i + \alpha_3 P_i + \phi PeerDP2000_i + \varepsilon_i$$

where

$DP2003_i$	$\sim$	indicator that worker $i$ draws DP in 2003
$PeerDP2000_i$	$\sim$	participation rate among $i$ 's peers in 2000
$X_i$	$\sim$	vector of 1999 characteristics of worker $i$
$N_i$	$\sim$	vector of 1995 characteristics of $i$ 's neighborhood and municipality
$P_i$	$\sim$	vector of 1995 characteristics of $i$ 's peer group
$\varepsilon_i$	$\sim$	error term with mean zero

The parameter of interest in equation (1) is  $\phi$ , which captures the impact of DP participation in 2000 among  $i$ 's peers on  $i$ 's propensity to draw DP by 2003. Note that since peer groups consist of persons working in 1995, the covariate of interest ( $PeerDP2000_i$ ) captures the DP entry rate of one's peers over 1995-2000.

Assuming  $PeerDP2000_i$  is independent of unobservable determinants of workers' DP entry decision ( $\varepsilon_i$ ), standard (non-IV) estimation of equation (1) will provide an unbiased estimate of  $\phi$ . However, the plausibility of this assumption is undermined by several potential problems.<sup>12</sup>

First, because individuals self-select into neighborhoods, it is possible that neighbors are similar in terms of their probability of becoming disabled or their distaste for work, yielding higher DP participation rates in some neighborhoods than others. Second, workers within a given neighborhood are

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<sup>12</sup> Manski (1993, 1995) provides a more complete and general analysis of the *reflection problem* in identifying social interaction effects. Our discussion of the identification issues is intended to address issues relevant in the context of disability application and participation.



similar in terms of the economic environment in which they work and/or search for work. Third, the DP screening process applied to applicants could vary across different locales affecting DP entry rates across neighborhoods. Each of these stories could explain why we might expect a positive within-neighborhood correlation of DP entry behavior even in the absence of social interaction effects. Unless these potential confounders are fully addressed by the included covariates, we might expect a *positively-biased* estimate of  $\phi$  when equation (1) is estimated using standard (non-IV) approaches. Notably, randomly assigning workers to neighborhoods would alleviate only the self-selection bias, not the other two sources of bias, which highlights the difficulty in generating plausible estimates of social interaction effects.

In contrast, the dynamic nature of our model raises potential sources of *negative* bias in estimates of  $\phi$ . First, randomness in the timing of DP entry among those entering can be a source of negative bias. To see this, consider a worker who might have entered DP either before 1999 or after 2000. If the worker enters DP earlier rather than later, he contributes to a higher  $PeerDP2000_i$  but the peer entry rate going forward is less than what we would otherwise measure. Therefore, in the absence of social interaction effects (and assuming away other potential sources of bias), we would expect standard estimates of  $\phi$  to be negative.

Second, if social interaction effects do exist, some of the residual variation in  $PeerDP2000_i$  potentially captures out-of-equilibrium variation in DP entry behavior. For instance, suppose that in the period preceding our analysis, workers in different neighborhoods were subject to varying shocks that affected DP entry. As a result, some of the residual variation in  $PeerDP2000_i$  could reflect social spillovers from these past shocks, as the peer DP participation rate converges to a new equilibrium. Provided that peer groups fully equilibrate from pre-1995 shocks in the 1995-2000 period, the out-of-equilibrium variation in  $PeerDP2000_i$  is not expected to influence DP entry going forward, leading to attenuation bias in standard estimates of  $\phi$ . In the absence other sources of bias, we would therefore expect standard estimates of  $\phi$  to understate the magnitude of social interaction effects, with the amount of bias determined by the extent that the residual variation in  $PeerDP2000_i$  is due to the indirect spillovers from past shocks rather than the direct effect of recent shocks.

In light of these potential problems, non-IV estimators of  $\phi$  are unlikely to provide an unbiased estimate of the social interaction effect. Moreover, we have no way of knowing a priori whether positive or negative sources of bias will dominate.

## 4.2. Instrumental Variable Approach

To address these numerous sources of bias, our approach for estimating  $\phi$  exploits recent and plausibly exogenous shocks affecting  $PeerDP2000_i$ . We can then identify social interaction effects by looking at how the shocks affecting a worker's peers subsequently affect that worker's likelihood of entering DP. Our strategy is similar to the “partial population intervention” approach advocated by Moffitt (2001).

Our strategy specifically uses recent plant downsizing events to instrument for the DP participation rate of a worker's peers ( $PeerDP2000_i$ ). This strategy hinges on two facts about disability participation. First, plant downsizing is a strong predictor of a worker's likelihood of entering disability in Norway, as previously established in Rege, Telle and Votruba (2007).<sup>13</sup> Second, disability participation is “sticky,” in the sense that participants rarely exit the system.<sup>14</sup> As a result, downsizing within peers' plants of employment is expected to increase the entry rate of peers onto DP, and this effect persists over time even in the absence of social interaction effects. Provided that downsizing in peers' plants of employment is independent of unobserved determinants of DP entry, the sources of positive bias discussed above would be alleviated. Our attention to recent shocks addresses the (negative) bias resulting from out-of-equilibrium changes in peer participation rates and randomness in the timing of DP entries.

The logic underlying our IV strategy is fairly straightforward. The direct effect of plant downsizing on DP participation can be captured by inclusion of individual-level plant downsizing covariates. In the aggregate, peer groups disproportionately exposed to plant downsizing events will subsequently demonstrate an increase in DP participation relative to peer groups in which fewer peers experienced plant downsizing. If social interaction effects exist, we should observe a relative increase in DP entry among workers in disproportionately hit peer groups independent of a worker's own downsizing experience.

Operationally, we implement a two stage linear probability model (2SLS).<sup>15</sup> The first stage equation predicts the DP participation rate among  $i$ 's peers at the end of 2000:

$$(2) \quad PeerDP2000_i = \beta_0 + \beta_1 X_i + \beta_2 N_i + \beta_3 P_i + \beta_4 PeerPDR_i + \nu_i$$

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<sup>13</sup> See also Røed and Fevang (2007) and Huttunen, Møen and Salvanes (2006) for how downsizing and organizational change affect workforce participation.

<sup>14</sup> Less than 1% per year (Annual Statistical Yearbook 2003, Norwegian National Insurance Administration).

<sup>15</sup> Results for alternative specifications are also presented.

where  $PeerPDR_i$  is a vector characterizing the exposure of  $i$ 's peers to plant downsizing events between 1995 and 1999.<sup>16</sup> The second stage equation, modified from equation (1), predicts the likelihood that a worker who is employed in 1999 draws disability in 2003:

$$(3) \quad DP2003_i = \alpha_0 + \alpha_1 X_i + \alpha_2 N_i + \alpha_3 P_i + \phi \widehat{PeerDP2000}_i + \varepsilon_i$$

where  $\widehat{PeerDP2000}_i$  is the predicted value of the peer DP participation rate from estimation of the first-stage equation.

Under the assumption that the plant downsizing experiences of a worker's peers are independent of the unobservable determinants of DP entry, 2SLS will provide consistent estimation of  $\phi$ . There are several reasons why this independence assumption may be problematic. First, peers' plant downsizing experiences could be correlated with a worker's own plant downsizing experience, either in the past or going forward. We address this concern through robustness tests, investigating whether our estimate is sensitive to inclusion of covariates capturing a worker's past (1995-1999) and future (1999-2003) plant downsizing experiences. Second, local plant downsizing events may be correlated with a decline in economic opportunities or future job prospects even for individuals in non-downsizing plants. Again, we can test whether our estimate is sensitive to inclusion of variables meant to proxy for such things, such as changes in the local unemployment rate. Finally, plant downsizing may be concentrated in neighborhoods populated with persons having generally higher propensities to draw sickness-related benefits. If so, we would expect peer downsizing rates to be correlated with rates of sick money and DP use prior to the downsizing events. The richness of our data allows us to test this possibility as well.

### 4.3. Interpreting the Social Interaction Coefficient and Deriving the Social Multiplier

Provided our empirical strategy allows enough time (four years) for each peer group to equilibrate from recent downsizing events, our IV approach provides a clean and policy relevant interpretation of the estimated social interaction coefficient  $\phi$  in terms of the social multiplier. To see this, suppose  $x$  fraction of a peer group is induced to enter DP as result of a recent (policy, economic, health or other) exogenous shock. Our estimate of  $\phi$  suggests that another  $\phi x(1-x)$  share of the peer group will subsequently enter DP as a result of social interaction effects. Thus, the *total* effect of the shock is an increase in the DP rate

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<sup>16</sup> We use peers' DP rate in 2000 as our covariate of interest instead of the rate in 1999, as plant downsizing over 1995-1999 is a stronger predictor of DP use in 2000 than in 1999. We attribute this to the lengthy application approval process as well as the possibility that responses to downsizing events might not be immediate.

within the group by  $x + \phi x(1 - x)$ . The social multiplier can therefore be estimated as  $1 + \phi(1 - x)$ , reasonably approximated as  $1 + \phi$  for sufficiently small shocks.

For completeness, we note two obvious problems with this estimate of the social multiplier. First, our results indicate that social interaction effects take longer to fully materialize than our estimation model (and data) allow. Second, it is conceivable that part of the social interaction effect materializes prior to 1999. In either case, we would expect  $\phi$  to be biased downwards. We therefore interpret our estimate of  $\phi$  (and the estimated social multiplier) as providing a lower bound on the social interaction effect in DP use.

## 5. Dataset Description

Our analysis utilizes a database provided by Statistics Norway called *FD-trygd*. *FD-trygd* includes a rich longitudinal dataset containing records for every Norwegian from 1992 to 2003. The variables captured in this dataset include individual demographic information (sex, age, marital status, number of children), socio-economic data (years of education, income, wealth), current employment status (full time, part time, minor part time, self-employed), industry of employment (if employed), indicators of participation in any of Norway’s welfare programs, and geographic identifiers for municipality and neighborhood of residence.

In addition, *FD-trygd* contains records for employment “events” since mid-1995. These events, captured by individual and date, include entry and exits into employment, changes in employment status (full time, part time, minor part time), and changes in plant and firm of employment. These employment events are constructed by data analysts at Statistics Norway from raw employment spell records submitted by employers, and verified against employee wage records (not available to us) to ensure the validity of each spell and to eliminate records pertaining to “secondary” employment spells.<sup>17</sup>

From these two data sources, four set of variables were created for use in our analysis, described below. The covariates used in our estimation models are described in greater detail in Appendix A.

### 5.1. Plant Downsizing Variables

Based on the employment records, we constructed plant-level employment counts at the end of years 1995, 1999 and 2003. The counts were constructed as measures of full-time equivalents (FTEs), with

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<sup>17</sup> If an individual was employed in multiple plants at a given time, primary employment was determined from employment status and recorded income from each source of employment.

part time and minor part time employment measured as 0.67 and 0.33 FTEs, respectively. Excluded from these counts were any persons identified in *FD-trygd* as self-employed or receiving assistance that should have precluded full time work (those receiving unemployment benefits, a rehabilitation pension or a disability pension). Plant-level FTEs were then used to construct measures of plant downsizing over two periods of time: from 1995 to 1999 and from 1999 to 2003. The measures, which we refer to as the “plant downsizing rate” (PDR), capture the percent decline in FTEs over the period. For instance, plants that fully closed over a given period were recorded as having a PDR=1 for that period; plants with FTE counts declining by 50 percent were recorded as having PDR=0.5. Plants that grew over a given period were recorded as PDR=0 for that period.

As our empirical strategy relies on the power of plant downsizing events to predict subsequent entry onto disability, we choose to focus on downsizing events in reasonably large plants. Specifically, the PDR variable was set to zero for workers employed in plants with fewer than 5 FTEs in the baseline year. Approximately 11 percent of workers were in plants of this size in 1995.

## 5.2. Worker Sample and Characteristics

Our analytic sample consists of native Norwegians age 45-63 employed either full time or part time in 1999, and also employed full time or part time in 1995. We chose to focus on older workers since these demonstrate the highest rates of DP entry. The upward age limit was imposed to ensure that none of our sample would be eligible for the normal retirement pension in 2003.<sup>18</sup> Excluded were any workers identified as self-employed or receiving assistance that should have precluded full time work (those receiving unemployment benefits, a rehabilitation pension or a disability pension), as well as any receiving social assistance. We excluded those employed in small plants (<5 FTEs) in 1999, for the purpose of controlling for the PDR of a worker’s current plant going forward (over 1999-2003). We also limited our sample to those residing in a neighborhood in 1995 that contained at least 10 workers age 41-62 to ensure that each person in our sample had a reasonable number of “peers” under our definition of peer groups (discussed below). Finally, we omitted 907 workers who had received a disability pension any time between 1995 and 1999, as well as one worker missing income/wealth variables in 1999. The resulting dataset consists of 378,148 workers residing in 10,209 different 1995 neighborhoods.

Variables capturing individual socio-economic characteristics were constructed based on records for 1999. These variables include age, sex, education, marital status, number of children, personal income, other household income, net household wealth and an indicator for receipt of a widow(er) pension. Employment-related variables include an indicator for part time status, tenure at

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<sup>18</sup> The age of eligibility for the normal retirement pension is 67.

current firm, plant size in 1999, and fourteen industry indicators.<sup>19</sup> The PDR of the worker's 1999 plant (1999-2003) was captured, as well as the past PDR (1995-1999) for the worker's 1995 plant of employment. Personal income and household wealth in 1995 was also captured, allowing us to control for the effect of changes in the workers' economic standing.

Our outcome of interest is an indicator variable capturing whether the worker received either temporary or permanent DP at the end of 2003, with the one caveat. For workers who died or emigrated prior to 2003 and those drawing an early retirement pension<sup>20</sup> in 2003, the indicator was set to one if the worker had entered DP prior to the event occurring. In sum, 6.9 percent of our sample received DP in 2003 (or prior to the events described above). Summary statistics for the remaining individual-level variables are presented in Table 1 (panel A).

### **5.3. Peer Groups and Characteristics**

As described in our empirical strategy, we define peer groups based on age, neighborhood of residence (in 1995) and employment status. Specifically, neighbors are included in a worker's peer group if they were age 41-62 and employed full time or part time in 1995. The upward age limit was imposed to ensure that peers were not eligible for the normal retirement pension in 2000. We defined peer groups based on 1995 neighborhoods of residence in case local downsizing events influenced worker mobility. If so, defining peer groups based on 1999 neighborhood of residence could lead to estimation bias through neighborhood self-selection.

Similar socio-economic and employment variables as those described for the worker sample were constructed at the peer group level, using records for 1995. Summary statistics for these characteristics are presented in Table 1 (panel B). Continuous variables were converted to categorical variables to create the peer-level covariates used in our estimation models. For instance, the age and sex composition of a worker's peers was captured as the fraction of peers in 14 age-sex categories (three-year age intervals interacted with sex). Peers' income and wealth were each captured as the fraction of peers in 6 categories defined based on the 10<sup>th</sup>, 25<sup>th</sup>, 50<sup>th</sup>, 75<sup>th</sup> and 90<sup>th</sup> percentiles for the distribution of the relevant variable over the full sample of peers. Additional program participation variables were created for the fraction of peers on social assistance, receiving sick money at the end of 1995 or having received sick money at any time in 1995.

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<sup>19</sup> Coded based on major categories in the Classification of Economic Activities in the European Community (NACE), with certain categories combined due to small sample sizes (agriculture, hunting and forestry was combined with fishing; activities of households was combined with other community, social and personal service activities; extra-territorial organizations and bodies was combined with public administration and defense).

<sup>20</sup> In some firms, workers satisfying specific work history requirements can qualify for an early retirement pension (AFP) at age 62.

Peer level PDR variables were created based on the level of downsizing in each peer's plant over 1995-1999. Four peer-level PDR variables were constructed capturing the fraction of peers employed (in 1995) in plants downsizing 10-30, 30-60, 60-100 and 100 percent.<sup>21</sup> These categories were found in Rege, Telle and Votruba (2007) to capture the individual-level effects of downsizing in a reasonably parsimonious way.

Finally, the DP rate of each worker's peers was constructed as the fraction of peers on permanent or temporary DP in 2000. As in the worker sample, we included in this fraction any peers who received DP prior to dying, emigrating or drawing an early retirement pension in 2000. Over our sample of workers, the mean participation rate of the workers' peer groups was 7.4 percent in 2000.

#### **5.4. Other Municipal and Neighborhood Level Characteristics**

We created additional variables to capture characteristics of the 1995 municipality and neighborhood of residence thought to potentially influence DP entry behavior. These include total native population; fraction of immigrants; fraction of natives age <18, 18-41 and  $\geq 62$  years old; mean income and wealth; and unemployment rate.<sup>22</sup> Additional variables capture the fraction of neighborhood and municipality residents, age 41-62 in 1995, in nine mutually exclusive "status" categories: receives permanent disability, receives temporary disability, receives rehabilitation pension, receives day money (unemployment), unemployed without receiving day money, self-employed, employed full time, employed part time, employed minor part time.<sup>23</sup> Summary statistics for these variables are presented in Table 1 (panel C).

## **6. Empirical Results**

### **6.1. Preliminary First Stage Results**

As discussed in Section 4, our IV strategy hinges on the fact that plant downsizing events affect individual DP entry, so that peers' exposure to downsizing can be used to predict the DP entry rate of one's peers. The individual-level effect of downsizing on DP entry was previously demonstrated in Rege, Telle and Votruba (2007), which also found substantial variation in this effect across industries.

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<sup>21</sup> Formally, the ranges of each downsizing category are [10%, 30%), [30%, 60%), [60%, 100%), and [100%], with the last category capturing full closures.

<sup>22</sup> The income, wealth and unemployment rate variables were calculated over natives age 22-67. For calculating the unemployment rate, the "employed" were counted as those working full time or part time, and the "unemployed" were counted as those neither working nor self-employed and having received unemployment benefits or registered as "looking for work" in the past year.

<sup>23</sup> A tenth (omitted) status category consists of persons neither employed nor receiving DP, rehabilitation pension or day money. We distinguish between this group and those "unemployed without receiving day money" based on whether the individual had registered as "looking for work" in the past year.

For the purpose of specifying our first-stage model, we therefore allow the effect of peers' downsizing exposure to vary depending on the industry of the "exposed" peers.

Table 2, Panel A reports linear (OLS) estimates of the effect of peers' exposure to downsizing events on the peer DP rate in 2000 over workers in our sample, analogous to our first stage model (equation 2). Each PDR covariate in the model (56 total) reflects the fraction of a worker's peers employed in a particular industry in 1995 whose plant of employment downsized the given magnitude by 1999. Covariates capturing the individual, peer, neighborhood and municipal characteristics in Table 1 are included in this and all subsequent models.<sup>24</sup> Of particular note, a set of 56 covariates captures the fraction of one's peers employed in plants of a particular industry and size.<sup>25</sup>

While the majority of the estimated PDR coefficients in the first stage model are positive (see Table 2), there is substantial variation in these estimates. Twenty-one of the estimates are actually negative in sign, with one of these (marginally) significant.<sup>26</sup> The aggregate predictive power of the PDR covariates is quite low, producing an F-statistic of 2.29. As a result, including the full set of PDR covariates in the instrument set raises a well-known set of "weak instrument" problems.<sup>27</sup> First, IV estimates based on the full set of potential instruments are expected to suffer from "finite sample bias" towards the OLS estimate. Second, the asymptotic assumptions underlying conventional hypothesis testing break down in the face of weak instruments, leading conventional standard errors to exaggerate precision of IV estimates. Third, if the instruments are not entirely exogenous, the expected bias is more severe when instruments are weak. Thus, potential failures of the identifying assumption are of greater concern when instruments are weak.

## 6.2. The Weak Instruments Problem and Alternative 2SLS Estimates

While asymptotic efficiency is obtained from including all valid instruments, the finite sample properties of IV estimates can be improved by selectively excluding valid instruments with weak power (Stock, Wright and Yogo, 2002). A number of "instrument selection" procedures have been suggested in the econometrics literature as means for addressing the weak instruments problem (e.g. Hall and Peixe 2002; Donald and Newey 2001), though a standard method has yet to emerge. For our analysis, we adopted a procedure to select among the set of potential instruments along the lines suggested by Donald and Newey (2001). Specifically, we sought to exclude potential instruments

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<sup>24</sup> See Appendix A for further details regarding the included covariates.

<sup>25</sup> Doing so addresses potential bias arising from the fact that we do not capture downsizing outcomes for peers originally employed in small plants.

<sup>26</sup> The PDR covariates demonstrating small or negative coefficients in the first stage model are generally those demonstrating smaller effects in similar models estimated at the individual level (see Rege, Telle and Votruba 2007).

<sup>27</sup> These problems are nicely surveyed by Stock, Wright and Yogo (2002). See also Bound, Jaeger and Baker (1995), Staiger and Stock (1997), and Hahn and Hausman (2003) for important contributions to this literature.



when their inclusion increased the mean square error around the IV estimate, the criteria employed by Donald and Newey.

Following Donald and Newey, we constructed a sequence of candidate instrument sets  $\{Z_K\}$ , where  $K=\{1, 2, \dots, 56\}$  denotes the number of PDR covariates in each set. The set  $Z_1$  consists solely of the PDR covariate with the largest marginal  $R^2$  contribution to the first stage regression (conditional on the other covariates). Each subsequent set,  $Z_{K+1}$ , consists of the PDR covariates in  $Z_K$ , as well as one additional PDR covariate determined by the covariate with the largest marginal  $R^2$  contribution to the first stage regression (conditional on  $Z_K$  and the other covariates). Thus, each  $Z_K$  set roughly consists of the  $K$  potential instruments providing the greatest power in the first stage.<sup>28</sup>

The results from these alternative models were used to calculate the approximate finite-sample bias in the 2SLS estimator and the asymptotic variance around the estimator, from which the approximate mean square error was calculated. The formulas for doing so are presented in Appendix B. As shown in Figure 2, the approximate finite sample bias is negative in sign and growing in magnitude as weaker instruments are included in the instrument set. The 2SLS estimates produced using alternative instrument sets roughly coincide with the approximate bias. Together, these results suggest that, corrected for finite sample bias, our instruments generally produce estimates of  $\phi$  of about 0.61.

Figure 2 presents 2SLS estimates of  $\phi$  under alternative instrument sets  $\{Z_K\}$  for values of  $K \geq 4$ . The 2SLS estimates range in magnitude from 0.61 when a restrictive set of instruments ( $K=6$ ) is employed down to roughly 0.45 when fuller sets of peer downsizing covariates are included as instruments ( $K \geq 40$ ). While not monotonic, the 2SLS estimates decline in a fairly linear fashion as progressively weaker instruments are added, consistent with finite sample bias towards a smaller OLS estimate.

Figure 3 plots the approximate mean square error associated with 2SLS estimates under alternative instrument sets. As progressively weaker instruments are added to the model, the bias of the 2SLS estimator increases but the asymptotic variance around the estimator decreases. Thus, in choosing among candidate instrument sets, we are essentially choosing between estimators that are less biased but less precise versus those that are more biased but more precise. Our calculations indicate that the approximate mean squared error around the 2SLS estimator is minimized when the  $Z_{14}$  instrument set is employed. For the remainder of our analysis, we therefore concentrate on IV

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<sup>28</sup> Due to the large number of potential instruments, an exact application of Donald and Newey's approach was not attempted and our approach varies in a number of respects. First, due to the large number of candidate instruments, we required that the sequence of candidate instrument sets be nested in one another (i.e.  $Z_1 \subset Z_2 \subset \dots \subset Z_{56}$ ). Second, alternative sets of instruments were constructed based solely on the power contributed by a candidate instrument in the first stage, rather than grouping potential instruments for *a priori* reasons.

results using as instruments the 14 peer PDR covariates demonstrating the greatest power in predicting the peer DP rate in 2000.

Panel B of Table 2 presents OLS coefficients from the first stage model using our preferred set of instruments ( $K=14$ ). The exclusion of weaker instruments from this model, cf. Panel A of Table 2, had only a modest effect on the coefficients for the included instruments. While the F-statistic (7.07) is substantially larger than that produced using the full set of instruments (2.29), it fails to reach levels where the weak instrument problem can be safely ignored (Staiger and Stock 1997). Thus, IV estimates under our preferred instrument set are still expected to suffer (modest) finite sample bias towards the OLS estimate, and conventional standard errors potentially understate the true variance around these estimates.

### 6.3. Main Results

The main results from our analysis are presented in Table 3. For comparison purposes, the first two columns report non-IV estimates of the social interaction effect. Estimated under a linear probability specification via OLS (column 1), our estimate suggests a one percent increase in the 2000 peer DP rate predicts a modest 0.07 percentage point increase in the subsequent entry rate (to 2003) of workers employed at the end of 1999. A probit specification produced mean marginal effect estimates about 20 percent smaller. Consistent with our previous discussion, the non-IV estimates of  $\phi$  are much smaller than the alternative 2SLS estimates in Figure 2. Thus, the potential positive bias resulting from correlated unobservables within groups appears to be dominated by the negative bias arising from dynamic aspects of our model (i.e. randomness in the timing of DP entries and the equilibration of peer DP rates from past shocks). Put another way, our estimate of  $\phi$  increases substantially when specifically identified off of recent shocks to the peer DP rate.

Columns 3-6 provide various IV estimates employing our preferred instrument set. Our 2SLS estimate is the same as that depicted visually in Figure 2 (for  $K=14$ ) and suggests that a one percentage point increase in the 2000 peer DP rate due to recent downsizing events increases the subsequent entry rate (1999-2003) of workers by 0.5 percentage points, an 7.2 percent increase relative to the aggregate rate of entry. Estimating our IV model using limited information maximum likelihood (LIML)<sup>29</sup> had little effect on our estimate (see column 4), as did estimation using two-step feasible generalized method of moments (results not shown). Employing Nagar's (1959) bias-corrected 2SLS model, the estimated  $\phi$  increases about 12 percent. Across each of these specifications, standard tests of

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<sup>29</sup> LIML estimators are known to be less biased than 2SLS but suffer from larger small sample variation (Hahn, Hausman and Kuersteiner, 2004).

overidentifying restrictions passed easily. In an IV-Probit specification, the estimated mean marginal effect is about 13 percent smaller than suggested by the linear models.

As mentioned earlier, the conventional standard errors reported for our IV estimates should be interpreted with caution as they potentially overstate the precision of our estimates due to the weakness of our instruments. To evaluate the extent of this problem, we compared the conventional confidence interval around our 2SLS estimate to that calculated using the “conditional likelihood ratio” approach developed by Moreira (2003).<sup>30</sup> There was very little difference between the two confidence intervals when evaluated at either the 95 percent or 99 percent levels, suggesting that the conventional standard errors provide a reasonably accurate gauge of estimate precision.

For comparison, columns 7-10 report IV results using the full set of peer PDR covariates as instruments. As anticipated by Figure 2, the 2SLS, LIML and IV-Probit estimates are modestly smaller than before, a result consistent with increasing finite sample bias, while the bias-corrected 2SLS estimate is somewhat larger. Tests of overidentifying restrictions fail in each of these models, another reason we concentrate on the findings using our preferred instrument set.

#### **6.4. Robustness Tests**

The identifying assumption in our IV approach is that the plant downsizing experiences of a worker’s peers occur independently of unobserved determinants of DP participation. Table 4 presents the results of robustness checks to test the validity of this assumption. For comparison, results from our 2SLS model (Table 3, column 3) are repeated in column 1.

An important concern for our identifying assumption is that exposure to downsizing is correlated across peers, who are frequently employed in the same plants. As a result, our IV estimate could reflect a delayed reaction to one’s own downsizing experience. If so, controlling for the past PDR (over 1995-1999) in workers’ 1995 plants would be expected to reduce the estimated social interaction coefficient.<sup>31</sup> As indicated in column 2, controlling for the past PDR of workers’ 1995 plants has negligible impact on the 2SLS estimate, despite adding significant power to the model ( $p < .0001$  for F test of joint significance).

Peers’ downsizing events could also be indicative of declining local labor market conditions, which could influence disability entry going forward, biasing our estimate upwards. Columns 3-5 include additional covariates expected to capture changes in a worker’s labor market conditions. In column 3, we

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<sup>30</sup> For the purposes of this comparison, the model was estimated under the assumption of independent, homoskedastic errors. As currently available in Stata®, the conditional likelihood ratio test statistics can only be calculated under this assumption. Murray (2006) cites Moreira’s approach as “state of the art for hypothesis testing with weak instruments” (p126).

<sup>31</sup> Similar to the PDR instruments, the individual covariates for own plant downsizing rate consist of interactions of the four PDR categories with industry (56 dummies variables).

include covariates capturing the PDR of the worker's 1999 plant going forward (over 1999-2003). In column 4, we include covariates capturing changes in the workers' personal income and household wealth since 1995. In column 5, we add county indicators<sup>32</sup> and covariates capturing the 1999 unemployment rate and mean income in each workers' 1995 neighborhood and municipality. Each additional set of covariates contributes significant power to the model ( $p < .0001$ ), but has negligible effect on the 2SLS estimate with the exception of the last, when the estimate increases modestly.

The remaining robustness checks address the concern that our measure of downsizing is a fairly crude measure of individual workers' exposure to employment shocks. Workers who switched plants over 1995-1999 are a particular concern in this regard, since they may have been exposed to downsizing in their subsequent plants, or may have been laid off from a plant that subsequently increased employment. Column 6 therefore estimates the 2SLS model excluding workers who switched plants. Column 7 reflects an even more conservative approach, restricting the sample to non-switchers in stable or growing plants over 1995-1999. We perceive this last model as a particularly strong test of social interaction effects, since the sample excludes all workers directly affected by the downsizing shocks used to identify our estimate. Interestingly, the 2SLS estimate increases somewhat in magnitude when plant switchers are excluded, perhaps reflecting that social interaction effects are stronger for workers with more stable employment. Indeed, it seems reasonable to think that "stable" workers might have stronger social ties to their neighbors, though we have no way of confirming this. Further restricting our sample to those in stable or growing plants had only a small effect on our estimate. Therefore, it seems unlikely that our estimate reflects a bias from unobserved past employment shocks that are correlated with the peer downsizing instruments.

## 6.5. Specification Tests

In this section, we explore the possibility that variation in pre-existing unobservables is potentially correlated with the variation in  $PeerDP2000_i$  explained by our instruments. We primarily have two unobservables in mind, which could be labeled broadly as "propensity for work" and "propensity for drawing disability-related benefits." For instance, if workers with lower propensities for work are those with peers disproportionately exposed to downsizing events, this would bias IV estimates of the social interaction effect upwards. A number of plausible stories could lead to such a bias. Downsizing events might be concentrated in areas with generally poorer employment opportunities or in areas where attachment to the labor force is generally weaker. Alternatively, since workers sort themselves into neighborhoods for reasons not entirely observable, workers with weaker attachment to the labor force might self-select neighborhoods where plant-employment patterns are less stable. Similar stories could be

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<sup>32</sup> Norway is divided into 19 counties.

told that would lead to correlation between the explained variation in  $PeerDP2000_i$  and the unobserved propensity for drawing disability benefits.

In the following specification tests, we use observed outcomes prior to 1995 to proxy for the unobservable propensities for work and for drawing disability-related benefits, and estimate the “effect” of  $PeerDP2000_i$  on these outcomes. A significant coefficient represents a potential source of bias.

Table 5 presents two sets of results in this regard. Panel A reports 2SLS estimates for the “effect” of  $PeerDP2000_i$  on the probability a worker in our sample is employed full- or part-time at the end of each calendar year. Panel B reports analogous estimates for the probability of receiving sick money at the end of each year. Concentrating on results prior to 1995, downsizing-induced changes in  $PeerDP2000_i$  are positively correlated with labor force attachment and (weakly) negatively correlated with sick money use. As a result, we might expect our IV estimates to suffer from a negative bias. Notably, these relationships change signs after 1999. In particular, we find a strong significant effect of  $PeerDP2000_i$  on sick money use after 1999, consistent with the emergence of a negative effect of peers’ downsizing exposure on workers’ willingness to take up sickness-related benefits. One troubling finding in Table 5 is the marginally significant “effect” of  $PeerDP2000_i$  on employment in 1997. This finding could potentially reflect declining labor market opportunities among workers in peer groups disproportionately exposed to downsizing. However, if this were a serious source of bias, we would have expected our estimate to decline under the sample restrictions discussed above (Table 4, columns 6 and 7).

A potential problem with the specification tests presented in Table 5 is that the observed outcomes relate specifically to our sample of workers, who were required to be employed in both 1995 and 1999. As a result, variation in local labor market conditions or in worker tastes might not be captured in the outcomes for this select sample. To address this concern, Table 6 reports the results from similar specifications employing neighborhood-level outcomes. Specifically, we report 2SLS estimates for the “effect” of  $PeerDP2000_i$  on DP and labor force participation rates in a worker’s neighborhood prior to 1995. Importantly, these rates are based on the entire population of similarly-aged persons residing in the worker’s 1995 neighborhood, not the subset of employed neighbors used to define peer groups. Also, we exclude as covariates from these models the neighborhood-level covariates capturing the fraction of neighbors in different employment and program use categories since these are collinear with the outcomes being modeled. As reported in Table 6, we find no evidence that unexplained pre-existing differences in neighborhood rates of employment or DP use are correlated with the variation in  $PeerDP2000_i$  explained by our instruments.

Taking together, these results fail to indicate that pre-existing differences across individuals or neighborhoods present a serious source of bias in our estimation of social interaction effects.

## 6.6. Disability Pension Entry to Alternative Endpoints

While our social interaction estimate is robust to inclusion of covariates intended to capture changing conditions in the local labor market (Table 4, columns 3-5), these covariates fail to fully capture workers' *perceptions* regarding the local labor market. Workers in neighborhoods disproportionately affected by downsizing events could form poor impressions of their labor market opportunities, triggering an increased rate of DP entry by such workers, biasing our IV estimate upwards. Unfortunately, we have no way of directly testing whether peers' exposure to downsizing events affects workers' perceptions in this way.

As an indirect test, Table 7 presents 2SLS estimates of the social interaction effect altering the "endpoint" at which DP use is measured. If our IV estimate merely reflects workers' response to the psychological shock of observing local downsizing events, we would expect the DP entry responses to be fairly contemporaneous with the occurrence of those downsizing events. That is, we would expect the social interaction coefficient to "level off" rather quickly. Instead, we find no evidence that the DP effect has "leveled off" by 2003. While this result does not preclude a potential "psychological effect" bias, it does undermine the argument that our estimate is merely an artifact of this bias. Moreover, since the social interaction coefficient increases substantially over the last year for which we have data, this suggests that our estimate likely understates the full magnitude of the effect that would be observed in equilibrium.

## 7. Conclusion

In this paper we estimate the magnitude of social interaction effects in disability pension participation among older workers in Norway. Specifically, we investigate how workers' propensity to draw DP benefits is affected by the disability participation of their "peers," defined as neighbors of similar age and employment status. To address issues of omitted variable bias, we use peers' exposure to plant downsizing events to instrument for peer rates of DP entry. To our knowledge, this is the first study to examine social interaction effects in disability participation.

Our linear probability estimates suggest that a one percentage point increase in the DP participation rate of previously employed neighbors increased the subsequent 4-year entry rate of employed workers by about 0.5 percentage points. Our non-linear (probit) IV estimate is somewhat smaller (0.44 percentage points), but remains large and highly significant. Moreover, several factors would suggest that our estimate is a lower bound on the magnitude of the social interaction effect. First, the relatively low power of our instruments contributes to finite sample bias towards the lower OLS estimate, as evidenced by our larger bias-corrected estimate. Second, we are able to follow workers for only a limited time following the downsizing events used to identify our estimate. Given

the substantial increase in the social interaction coefficient when DP use is measured to 2003 versus 2002, it is doubtful that peer rates of DP participation have fully equilibrated from the identifying shocks by 2003. Related to this, part of the social interaction effect is conceivably experienced prior to 1999, which would effectively reduce our IV estimate since DP entrants prior to 1999 are eliminated from our sample workers.

The causal interpretation of our social interaction estimate depends critically on the assumption that peers' exposure to downsizing events is independent of unobservable determinants of disability entry. *Ex ante*, one might reasonably expect downsizing events to be concentrated in neighborhoods with workers having higher pre-existing propensities for entering disability. However, we find no evidence that the variance in peer DP rates induced by peers' exposure to plant downsizing events is associated with the neighborhood rate of DP use prior to the downsizing events. Workers in peer groups disproportionately exposed to downsizing have *higher* rates of employment and *lower* rates of sick money use prior to the downsizing events suggesting, if anything, our estimate is bias downwards.

Alternatively, local downsizing events could adversely affect local labor market conditions, causing a rise in disability entry rates independent of any social interaction effect. Our robustness tests fail to provide support for such a bias. While we cannot entirely rule out the possibility that our estimate is contaminated by the psychological effect of observing local downsizing events, our estimates of the social interaction effect to different points in time shed doubt on this as a major source of bias.

Our results suggest that 1.5 provides a reasonable lower-bound estimate of the social multiplier in disability participation when measured at the level of Norwegian neighborhoods. As demonstrated in Glaeser, Sacerdote and Scheinkman (2003), the level of aggregation can greatly affect the size of estimated social multipliers. Moreover, Norway has a particularly homogeneous population, which could contribute to especially large social interaction effects. Nonetheless, our results suggest a social multiplier similar in magnitude to those estimated by Glaeser, Sacerdote and Scheinkman (2003) in college dorm rates of fraternity membership and county-level crime rates.

The large magnitude of social interaction effects has important implications for research in disability insurance participation. Social interaction effects could conceivably explain the large variation in SSDI participation across U.S. counties (McCoy et al. 1994). They could conceivably contribute to the sizable labor supply response to disability benefit increases observed in Canada (Gruber 2000), as well as the large SSDI response to the coal boom/bust observed in coal-producing states (Black et al. 2002). To the extent that social networks are defined along socio-economic lines, they could help explain the large increase in disability participation among low-skilled U.S. workers,

attributed in Autor and Duggan (2003) to the declining demand for low-skilled workers and an unforeseen increase in their disability benefit replacement rates. As a general empirical matter, the existence of large social interaction effects requires careful interpretation of estimates meant to capture the individual-level determinants of disability participation to the extent these determinants are concentrated within a particular social network. Such estimates likely exaggerate the individual-level importance of such determinants but under-estimate the full (aggregate) effect.

For policy-makers, our results lend empirical support to concerns about the potential development of welfare cultures arising from poorly designed disability insurance programs. From a social welfare perspective, the existence of large social interactions could dramatically affect estimates of the “optimal” earnings replacement rate, an issue that has received little attention in the program design literature.<sup>33</sup> The existence of social interaction effects would also strengthen arguments made by Autor and Duggan (2006) regarding the importance of developing screening procedures that minimize potential abuse of disability insurance programs. Finally, our results indicate that efforts to reduce the impact of economic shocks on disability entry (e.g. retraining programs, job search assistance) would, if effective, also reduce disability participation among persons not directly affected by those shocks.

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<sup>33</sup> Bound et al. (2004) provides a useful framework for evaluating the social welfare implications of disability benefit changes, but does not explicitly address the role of social interaction effects.



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## Covariate Details

The table below describes the exact covariates included our estimation models.

<b>Panel A: Individual-level characteristics (in 1999 unless otherwise indicated)</b>			
<b>Characteristic</b>	<b>Covariates</b>	<b>Analyses</b>	<b>Comments</b>
sex/age	indicator for female, plus third-order polynomial of age interested with sex	all	
personal income	third-order polynomial	all	includes all non-capital sources of income, including government transfers
other household income	third-order polynomial	all	total household income net of personal income
household wealth	third-order polynomial	all	
education	indicators for education $\leq 9$ , 13-15, and $\geq 16$ years	all	missing assigned to omitted category (10-12 years)
marital status	indicators for married, widowed, and divorced	all	missing assigned to omitted category (single)
number of children	indicators for 1, 2-3, and $\geq 4$	all	number reflects count of persons <18 years old in household
receives widow(er) pension	indicator	all	
employed part-time	indicator	all	
tenure at plant	indicators for 1-3, 3-5, and $\geq 5$ years	all	
1999 plant industry/size	indicators for plant industry (14 categories) and size (3 categories: 5-25, 25-100, and $\geq 100$ FTEs)	all	42 total, one omitted due to colinearity; sample excludes those in (1999) plants employing <5 FTEs
1999 plant industry/PDR	indicators for plant industry (14 categories) and 1999-2003 PDR (4 categories: 10-30%, 30-60%, 60-100%, and 100%)	Table 4, (3)-(7)	56 total
1995 plant industry/size	indicators for plant industry and size (4 categories: <5, 5-25, 25-100, and $\geq 100$ FTEs)	Table 4, (2)-(7)	56 total, one omitted due to colinearity
1995 plant industry/PDR	indicators for plant industry (14 categories) and 1995-99 PDR (4 categories: 10-30%, 30-60%, 60-100%, and 100%)	Table 4, (2)-(7)	56 total, set to zero if workers' 1995 plant employed <5 FTEs
$\Delta$ income/wealth, 1995-99	third-order polynomials for changes in personal income, other household income, and household wealth	Table 4, (4)-(7)	

<b>Panel B: Peer group characteristics (in 1995)</b>			
<b>Characteristic</b>	<b>Covariates</b>	<b>Analyses</b>	<b>Comments</b>
sex/age	fraction of peers in 14 sex-age categories (age categories: 41-44, 44-47, ..., 59-62)	all	14 total, one omitted due to colinearity
education	fraction of peers in three categories: $\leq 9$ , 13-15, and $\geq 16$ years of education	all	
marital status	fraction of peers in three categories: married, widowed, and divorced	all	
number of children	fraction of peers in three categories: 1, 2-3, and $\geq 4$ children in household	all	
receives widow(er) pension	fraction of peers receiving	all	
receives sick money	fraction of peers receiving	all	
received sick money in year	fraction of peers who received earlier in year (but not at end of year)	all	
receives social assistance	fraction of peers receiving	all	
personal income	fraction of peers in six categories, defined by 10 <sup>th</sup> , 25 <sup>th</sup> , 50 <sup>th</sup> , 75 <sup>th</sup> and 90 <sup>th</sup> percentile in sample distribution	all	one omitted due to colinearity
other household income	fraction of peers in six categories, defined by 10 <sup>th</sup> , 25 <sup>th</sup> , 50 <sup>th</sup> , 75 <sup>th</sup> and 90 <sup>th</sup> percentile in sample distribution	all	one omitted due to colinearity
household wealth	fraction of peers in six categories, defined by 10 <sup>th</sup> , 25 <sup>th</sup> , 50 <sup>th</sup> , 75 <sup>th</sup> and 90 <sup>th</sup> percentile in sample distribution	all	one omitted due to colinearity
employed part-time	fraction of peers	all	
tenure	fraction of peers with $\geq 1$ year of tenure in 1995 plant	all	
1995 plant industry/size	fraction of peers in 56 plant industry/size categories (defined same as in Panel A)	all	one omitted due to colinearity
1995 plant industry/PDR	fraction of peers in 56 plant industry/PDR categories (defined same as in Panel A)	--	defines full set of potential instruments
total count of peers	second-order polynomial	all	see text for definition

Panel C: Neighborhood-level characteristics (in 1995 unless otherwise indicated)			
Characteristic	Covariates	Analyses	Comments
total population	total population	total population	total population
fraction of immigrants	fraction of immigrants	fraction of immigrants	fraction of immigrants
Age	Age	Age	Age
mean personal income	mean personal income	mean personal income	mean personal income
mean other household inc	mean other household inc	mean other household inc	mean other household inc
mean household wealth	mean household wealth	mean household wealth	mean household wealth
unemployment rate	unemployment rate	unemployment rate	unemployment rate
employment/program status <sup>a</sup>	employment/program status <sup>a</sup>	employment/program status <sup>a</sup>	employment/program status <sup>a</sup>
1999 unemployment rate	1999 unemployment rate	1999 unemployment rate	1999 unemployment rate
1999 mean personal income	1999 mean personal income	1999 mean personal income	1999 mean personal income
1999 “small” neighborhood	1999 “small” neighborhood	1999 “small” neighborhood	1999 “small” neighborhood

<b>Panel D: Municipal-level characteristics (in 1995 unless otherwise indicated)</b>			
<b>Characteristic</b>	<b>Covariates</b>	<b>Analyses</b>	<b>Comments</b>
total population	total population	total population	total population
fraction of immigrants	fraction of immigrants	fraction of immigrants	fraction of immigrants
Age	Age	Age	Age
mean personal income	mean personal income	mean personal income	mean personal income
mean other household inc	mean other household inc	mean other household inc	mean other household inc
mean household wealth	mean household wealth	mean household wealth	mean household wealth
unemployment rate	unemployment rate	unemployment rate	unemployment rate
employment/program status <sup>a</sup>	employment/program status <sup>a</sup>	employment/program status <sup>a</sup>	employment/program status <sup>a</sup>
1999 unemployment rate	1999 unemployment rate	1999 unemployment rate	1999 unemployment rate
1999 mean personal income	1999 mean personal income	1999 mean personal income	1999 mean personal income
county	county	county	county

<sup>a</sup> For the purpose of creating the “employment/program status” covariates, all natives age 41-62 in 1995 were assigned to one of nine mutually exclusive categories, defined as:

- receiving permanent DP
- receiving temporary DP
- receiving rehabilitation pension
- receiving day money (unemployment benefits)
- self-employed
- employed full-time
- employed part-time
- employed minor part-time
- unemployed (without receiving day money)

The “unemployed” category captures persons neither currently employed nor receiving day money, but who were registered with the government as seeking employment in the past year. Thus, it is intended to capture those who are likely still in the workforce. (This group was combined with the “receiving day money” group for the purpose of constructing unemployment rate variables.) To ensure that the status categories were mutually exclusive, statuses were assigned in a stepwise fashion, such that assignment to an “earlier” category precluded assignment to a latter category (with the categories ordered as listed above).



## Calculating Approximate Bias and MSE

The following describes the formulas used for calculating the approximate bias and mean squared error (MSE) around the 2SLS estimates presented in Figure 2. Similar representations for these formulas exist in the literature, although these are often expressed for cases where the second stage model includes a single endogenous covariate.<sup>34</sup> These formulas have been modified to accommodate the presence of exogenous covariates and clustering of the error terms within neighborhood. Our notation follows that of Wooldridge (2002).

For a given instrument set ( $K$ ), the MSE around the 2SLS estimate can be written as:

$$(B1) \quad MSE_K(\hat{\beta}_{2SLS}) = Var_K(\hat{\beta}_{2SLS}) + \left( Bias_K(\hat{\beta}_{2SLS}) \right)^2$$

or, more succinctly,

$$MSE_K = Var_K + \left( Bias_K \right)^2$$

where  $Var_K$  denotes the variance around the 2SLS estimator and  $Bias_K$  denotes the finite sample bias.

Following Bound, Jaeger and Baker (1995), the finite sample bias in the 2SLS estimator can be approximated as:

$$(B2) \quad Bias_K \approx \frac{\sigma_{\varepsilon v}(K-2)}{\sum_{i=1}^N r_{iK}^2}$$

where  $\sigma_{\varepsilon v}$  denotes the covariance between the second stage and first stage residuals and  $r_{iK}$  denotes the expected change in the predicted value of  $PeerDP2000_i$  induced by the included covariates.

$$(B3) \quad r_{iK} = E(PeerDP2000|X_i, Z_{iK}) - E(PeerDP2000|X_i).$$

An estimate of  $r_{iK}$  can be derived as the residual from the regression of  $PeerDP\hat{P}2000_{iK}$ , the predicted value employing instrument set  $K$ , on the exogeneous covariates:

$$(B4) \quad PeerDP\hat{P}2000_{iK} = \hat{\delta}_K X_i + \hat{r}_{iK}.$$

The denominator in (B2) can therefore be estimated as sum of squared residuals from (B4).

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<sup>34</sup> E.g. Hahn, Hausman and Kuersteiner (2004).

To estimate the numerator, an estimate of  $\sigma_{\varepsilon v}$  can be calculated in typical fashion based on the estimated residuals from the first and second stage models. In our calculations, we estimate  $\sigma_{\varepsilon v}$  based on the K=56 model (full instrument set), holding this value constant for alternative K, in line with the procedure recommended Donaldson and Newey (2001). In doing so, differences in the approximate bias across different (K) estimators are driven entirely by differences in the number of instruments employed and the explanatory power of those instruments.

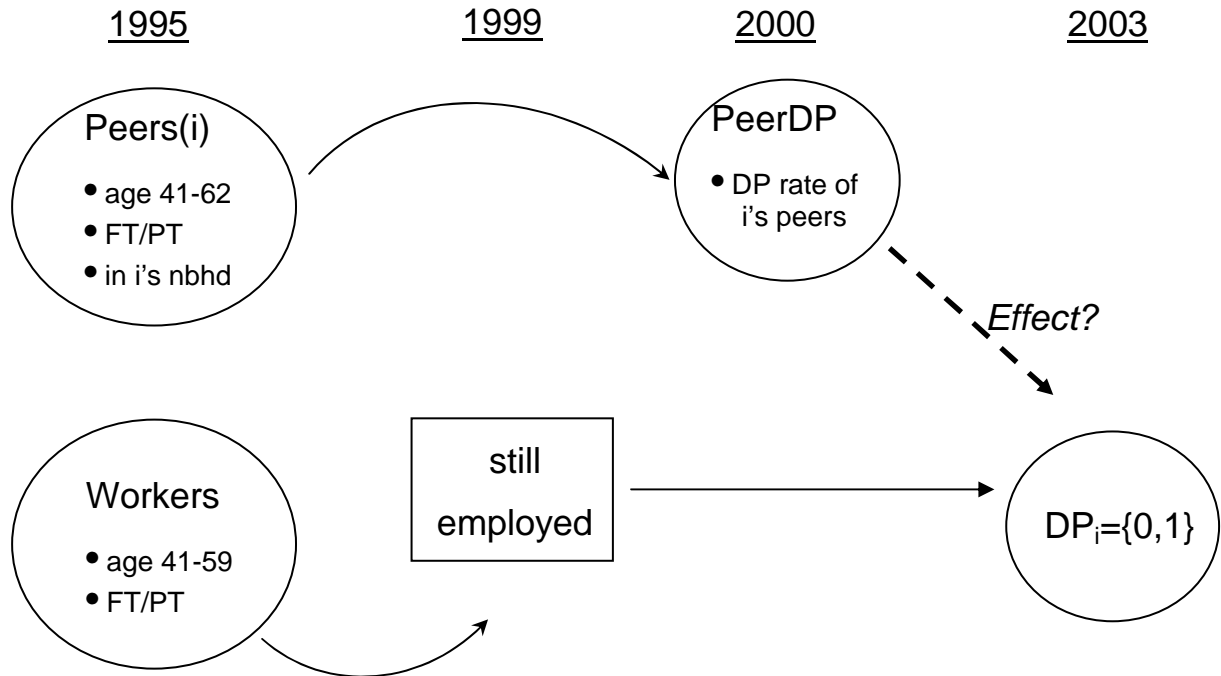
The variance around the 2SLS estimator is approximated by an estimate of its asymptotic variance. Adjusting for within-neighborhood clustering,

$$(B5) \quad \widehat{Var}_K \approx \frac{\sum_{j=1}^{N_j} \left( \sum_{i \in j} \hat{\varepsilon}_i \hat{r}_{iK} \right)^2}{\left( \sum_{i=1}^N \hat{r}_{iK}^2 \right)^2}$$

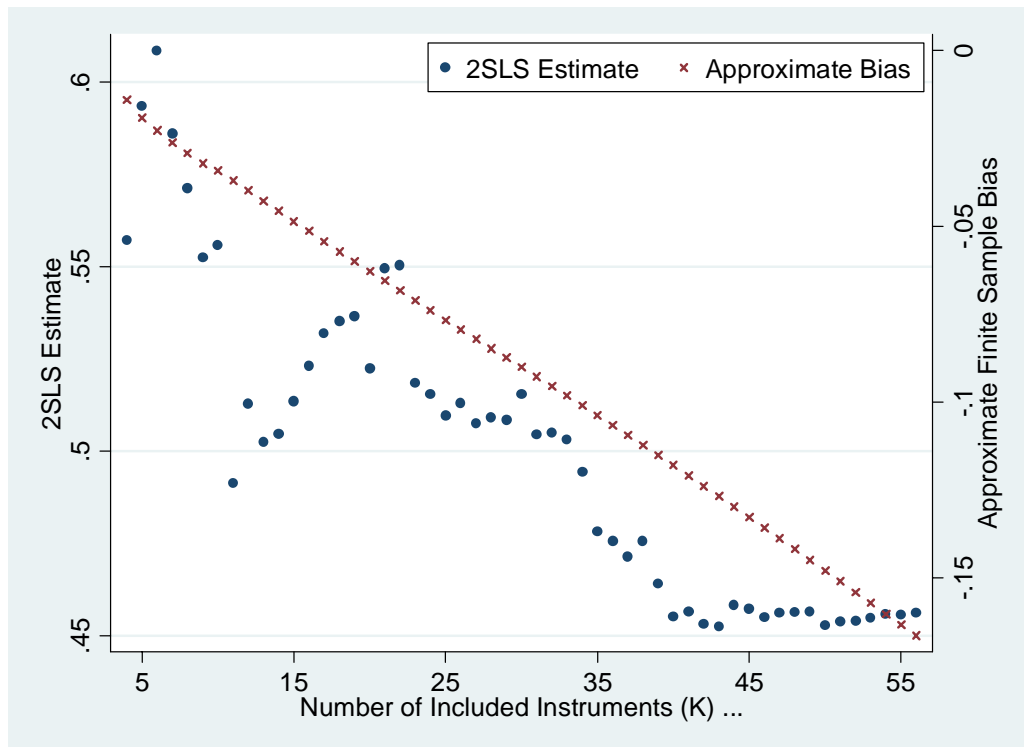
Again, the estimated second stage residuals  $(\hat{\varepsilon}_i)$  were derived from the K=56 model and held constant across alternative models, so that differences in the estimated estimator variance across models is primarily the result of differences in the predictive power afforded by different instrument sets.

## Figures and Tables

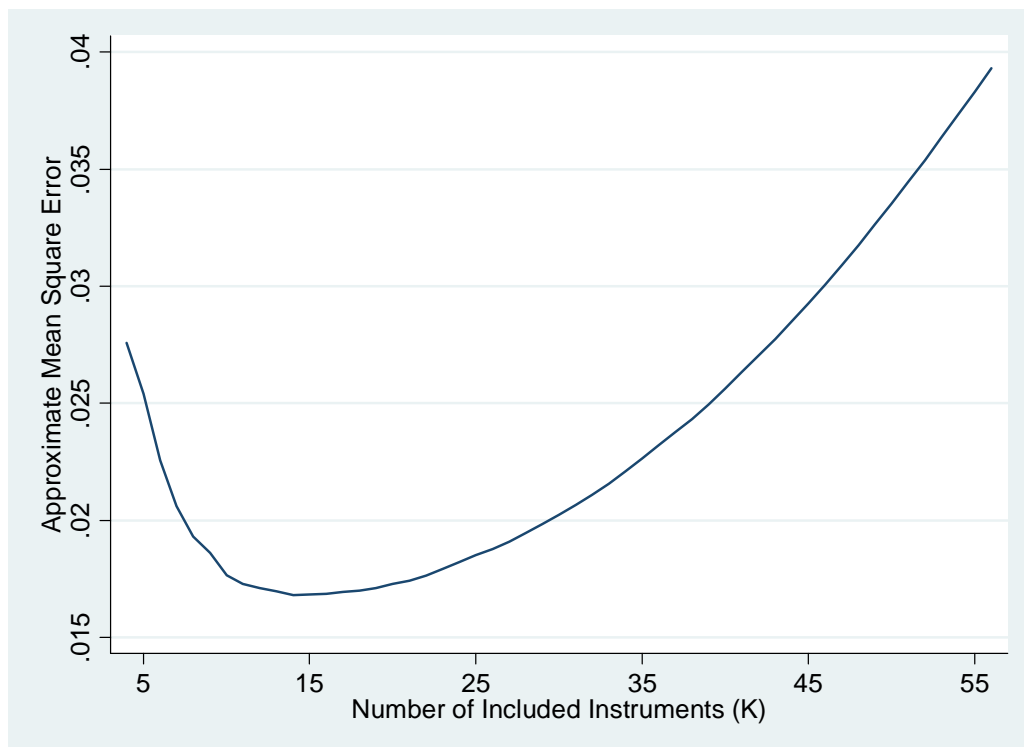
Figure 1: Empirical Strategy Timeline



**Figure 2: 2SLS Estimates under Alternative Instrument Sets**



**Figure 3: Approximate Mean Square Error of 2SLS Estimates**



**Table 1: Summary Statistics**

Panel A: Worker Characteristics (1999) <sup>a</sup>			
Characteristic	Fraction/Mean (s.d.)	Characteristic	Fraction/Mean (s.d.)
2003 DP utilization <sup>b</sup>	0.069	Industry	
Age	52.69 (4.715)	agriculture, fishing	0.004
Female	0.423	mining, oil	0.022
Education (years)		manufacturing	0.171
≤9 yrs	0.133	electric, gas, water	0.017
13-15 yrs	0.318	construction	0.055
≥16 yrs	0.195	wholesale/retail trade	0.106
Marital status		hotels, restaurants	0.010
married	0.726	transport, communic.	0.089
widowed	0.024	financial intermed.	0.040
divorced	0.138	real estate, business	0.068
Children <18 y.o.		public admin, defense	0.112
1	0.268	education	0.136
2-3	0.265	health, social work	0.144
≥4	0.012	other services	0.026
On widow(er) pension	0.015	1999-2003 PDR <sup>c</sup>	
Income/wealth		10-30%	0.168
personal income	315969 (169671)	30-60%	0.108
other HH income	279240 (586291)	60-100%	0.134
net HH wealth	320633 (2881228)	100%	0.060
Emp status: PT	0.089	1995-1999 PDR <sup>d</sup>	
Tenure		10-30%	0.185
1-3 yrs	0.208	30-60%	0.076
3-5 yrs	0.160	60-100%	0.127
≥5 yrs	0.579	100%	0.075
Plant size (FTEs)		1995 Income/wealth	
5-25	0.231	personal income	258467 (126669)
25-100	0.267	other HH income	222348 (285368)
≥100	0.318	net HH wealth	141959 (2616800)

<sup>a</sup> Characteristics measured at end of 1999 unless otherwise indicated.

<sup>b</sup> Includes workers entering DP prior to death, emigrating, or drawing early retirement.

<sup>c</sup> Measures decline in employment (FTEs) in worker's 1999 plant of employment.

<sup>d</sup> Measures decline in employment (FTEs) in worker's 1995 plant of employment, set to zero for plants with fewer than 5 FTEs in 1995.

**Table 1: Summary Statistics (cont.)**

Panel B: Peer Group Characteristics (1995) <sup>a</sup>			
Characteristic	Fraction/Mean (s.d.)	Characteristic	Fraction/Mean (s.d.)
2000 DP rate <sup>b</sup>	0.074 (0.045)	Plant size (FTEs)	
Age	49.75 (1.343)	5-25	0.248 (0.094)
Female	0.434 (0.070)	25-100	0.282 (0.087)
Education (years)		≥100	0.359 (0.142)
≤9 yrs	0.163 (0.091)	Industry	
13-15 yrs	0.286 (0.081)	agriculture, fishing	0.006 (0.018)
≥16 yrs	0.171 (0.102)	mining, oil	0.019 (0.035)
Marital status		manufacturing	0.167 (0.108)
married	0.744 (0.145)	electric, gas, water	0.015 (0.026)
widowed	0.021 (0.021)	construction	0.051 (0.041)
divorced	0.124 (0.080)	wholesale/retail trade	0.136 (0.063)
Children <18 y.o.		hotels, restaurants	0.013 (0.020)
1	0.193 (0.076)	transport, communic.	0.085 (0.052)
2-3	0.154 (0.082)	financial intermed.	0.036 (0.029)
≥4	0.007 (0.015)	real estate, business	0.065 (0.048)
On widow(er) pension	0.014 (0.017)	public admin, defense	0.106 (0.067)
On social assistance	0.005 (0.012)	education	0.117 (0.067)
On sick money	0.042 (0.030)	health, social work	0.151 (0.069)
Rec'd SM in year	0.118 (0.050)	other services	0.033 (0.030)
Income/wealth <sup>c</sup>		1995-1999 PDR <sup>c</sup>	
personal income	250928 (37088)	10-30%	0.173 (0.078)
other HH income	226370 (70577)	30-60%	0.081 (0.055)
net HH wealth	177065 (295593)	60-100%	0.129 (0.076)
Emp status: PT	0.124 (0.058)	100%	0.074 (0.050)
Tenure ≥1 yr	0.917 (0.043)	Peer group population	118.9 (115.8)

<sup>a</sup> Characteristics measured at end of 1995 unless otherwise indicated.

<sup>b</sup> Includes workers entering DP prior to death, emigrating, or drawing early retirement.

<sup>c</sup> Measures decline in employment (FTEs) in worker's 1995 plant of employment, set to zero for plants with fewer than 5 FTEs in 1995.

**Table 1: Summary Statistics (cont.)**

Panel C: Neighborhood and Municipality Characteristics			
Characteristic	Fraction/Mean (s.d.)	Characteristic	Fraction/Mean (s.d.)
Neighborhood (1995)		Municipality (1995)	
total population <sup>a</sup>	692.3 (607.7)	total population <sup>a</sup>	75828.0 (116071.9)
fraction immigrant	0.049 (0.058)	fraction immigrant	0.055 (0.045)
fraction <18 y.o. <sup>a</sup>	0.225 (0.065)	fraction <18 y.o. <sup>a</sup>	0.222 (0.031)
fraction 18-41 y.o. <sup>a</sup>	0.518 (0.074)	fraction 18-41 y.o. <sup>a</sup>	0.529 (0.025)
fraction ≥62 y.o. <sup>a</sup>	0.180 (0.090)	fraction ≥62 y.o. <sup>a</sup>	0.192 (0.036)
mean income <sup>b</sup>	174283 (32351)	mean income <sup>b</sup>	170159 (21851)
mean wealth <sup>b</sup>	35685 (128383)	mean wealth <sup>b</sup>	32770 (45732)
0 unemployment rate <sup>b</sup>	0.136 (0.061)	unemployment rate <sup>b</sup>	0.144 (0.046)
fract. emp'd FT <sup>c</sup>	0.546 (0.099)	fract. emp'd FT <sup>c</sup>	0.524 (0.075)
fract. emp'd PT <sup>c</sup>	0.076 (0.033)	fract. emp'd PT <sup>c</sup>	0.073 (0.017)
fract. emp'd MPT <sup>c</sup>	0.056 (0.029)	fract. emp'd MPT <sup>c</sup>	0.057 (0.017)
fract. self-emp'd <sup>c</sup>	0.071 (0.052)	fract. self-emp'd <sup>c</sup>	0.080 (0.039)
fract. perm. DP <sup>c</sup>	0.115 (0.061)	fract. perm. DP <sup>c</sup>	0.122 (0.032)
fract. temp. DP <sup>c</sup>	0.002 (0.005)	fract. temp. DP <sup>c</sup>	0.002 (0.002)
fract. rehab pension <sup>c</sup>	0.019 (0.016)	fract. rehab pension <sup>c</sup>	0.020 (0.007)
fract. day money <sup>c</sup>	0.043 (0.028)	fract. day money <sup>c</sup>	0.045 (0.016)
fract. unemployed <sup>c</sup>	0.012 (0.013)	fract. unemployed <sup>c</sup>	0.013 (0.004)
Neighborhood (1999)		Municipality (1999)	
mean income <sup>b,d</sup>	220396 (41567)	mean income <sup>b</sup>	216682 (29146)
unemployment rate <sup>b,d</sup>	0.075 (0.045)	unemployment rate <sup>b</sup>	0.079 (0.034)
<20 in neighborhood <sup>b</sup>	0.007 (0.083)		

<sup>a</sup> Calculated over native Norwegians.

<sup>b</sup> Calculated over natives age 22-67.

<sup>c</sup> Calculated over natives age 41-62.

<sup>d</sup> Set to missing if neighborhood no longer exists or contains fewer than 20 natives age 22-67 in 1999.

<sup>e</sup> Calculated over natives age 41-62 excluding those in worker's neighborhood.

*Table Notes:* N=378148. Sample consists of workers, age 45-63 in 1999, employed FT or PT in 1995 and 1999, excluding those in small 1999 plants (<5 FTEs), on social assistance in 1999, missing income/wealth variables in 1999, or having fewer than 10 persons in defined peer group (see text for definition).

**Table 2: Effect of Peer Plant Downsizing on Peer Rate of Disability Pension Utilization**

<b>Panel A: Full set of instruments</b>									
Dependent variable: Peer 2000 DP rate									
PDR	Agriculture, fishing	Mining, oil	Manufacturing	Electric, gas, water	Construction	Wholesale/retail trade	Hotels, restaurants		
10-30%	-.065 (.086)	.056 (.046)	.024 (.009)*	.008 (.035)	.014 (.028)	.022 (.021)	.061 (.060)		
30-60%	-.044 (.089)	-.003 (.040)	.001 (.012)	-.035 (.053)	.038 (.040)	.056 (.024)*	.011 (.066)		
60-100%	.166 (.105)	.178 (.036)**	.025 (.014) <sup>+</sup>	.120 (.053)*	.055 (.037)	-.002 (.029)	-.071 (.077)		
100%	.052 (.093)	.299 (.116)**	.020 (.023)	-.006 (.049)	.026 (.035)	.011 (.028)	.145 (.063)*		
PDR	Transport, communic.	Financial intermed.	Real estate, business	Public admin, defense	Education	Health, social work	Other services		
10-30%	-.019 (.030)	-.048 (.031)	.031 (.032)	.011 (.019)	.004 (.014)	-.001 (.013)	-.027 (.045)		
30-60%	.084 (.032)**	-.016 (.040)	-.040 (.046)	-.036 (.020) <sup>+</sup>	-.007 (.023)	-.008 (.020)	.009 (.055)		
60-100%	.062 (.023)**	.003 (.063)	.071 (.044)	.013 (.015)	.041 (.013)**	.009 (.013)	-.063 (.056)		
100%	.022 (.026)	-.002 (.067)	-.008 (.036)	.054 (.033) <sup>+</sup>	-.021 (.018)	-.019 (.017)	.047 (.056)		



**Table 2: Effect of Peer Plant Downsizing on Peer Rate of Disability Pension Utilization (cont.)**

<b>Panel B: Preferred set of instruments</b>									
Dependent variable: Peer 2000 DP rate									
PDR	Agriculture, fishing	Mining, oil	Manufacturing	Electric, gas, water	Construction	Wholesale/retail trade	Hotels, restaurants		
10-30%	--	--	.024 (.009)**	--	--	--	--		
30-60%	--	--	--	--	--	.047 (.023)*	--		
60-100%	.198 (.092)*	.167 (.033)**	.024 (.013) <sup>+</sup>	.124 (.051)*	--	--	--		
100%	--	.295 (.117)*	--	--	--	--	.141 (.060)*		
PDR	Transport, communic.	Financial intermed.	Real estate, business	Public admin, defense	Education	Health, social work	Other services		
10-30%	--	--	--	--	--	--	--		
30-60%	.082 (.031)**	--	--	-.042 (.019)*	--	--	--		
60-100%	.059 (.022)**	--	.068 (.043)	--	.045 (.011)**	--	--		
100%	--	--	--	.048 (.032)	--	--	--		

*Notes:* N=378148. Mean dependent variable=.074. Estimates in each panel reflect results from single OLS model, adjusted for peer group, neighborhood and municipality characteristics in 1995, and individual characteristics in 1999. Panel B limits the instrument set to minimize the approximate mean squared error around the IV estimate (see text for details). Conventional robust standard errors in parentheses, corrected for non-independent residuals within neighborhoods. +, \* and \*\* denote significance at the 10, 5 and 1 percent level.

**Table 3: Main Results: Effect of Social Interaction on Disability Pension Entry**

Dependent variable: 2003 DP utilization										
	No instruments		Preferred instrument set				Full instrument set			
	OLS (1)	Probit (2)	2SLS (3)	LIML (4)	BC-2SLS (5)	IV-Probit (6)	2SLS (7)	LIML (8)	BC-2SLS (9)	IV-Probit (10)
Peer 2000 DP rate	.069** (.012)	.461** (.089) [.055]	.504** (.123)	.506** (.124)	.568** (.144)	3.602** (.923) [.441]	.456** (.108)	.463** (.110)	.754** (.212)	3.388** (.982) [.414]
<i>First-stage results</i>										
F statistic			7.07	7.07	7.07	7.07	2.29	2.29	2.29	2.29
Partial R <sup>2</sup>			.0100	.0100	.0100	.0100	.0123	.0123	.0123	.0123
<i>Test of overidentifying restrictions</i>										
J/AR/Sargan statistic <sup>a</sup>			10.35	12.34	12.64		75.20	84.67	92.71	
p-value			.665	.500	.476		.037	.006	.001	
mean	.069	.069	.069	.069	.069	.069	.069	.069	.069	.069
N	378148	378148	378148	378148	378148	378148	378148	378148	378148	378148

*Notes:* All estimates adjusted for peer group, neighborhood and municipality characteristics in 1995, and individual characteristics in 1999. Conventional robust standard errors in parentheses, corrected for non-independent residuals within neighborhoods. <sup>+</sup>, \* and \*\* denote significance at the 10, 5 and 1 percent level. Mean marginal effect implied from probit models presented in brackets.

<sup>a</sup> Hansen J statistic reported for 2SLS, Anderson-Rubin statistic for LIML, and Sargan statistic for BC-2SLS.

**Table 4: Robustness Checks**

Dependent variable: 2003 DP utilization							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Peer 2000 DP rate	.504** (.123)	.500** (.125)	.504** (.125)	.505** (.125)	.525** (.138)	.677** (.187)	.653* (.260)
<i>First-stage results</i>							
F-statistic	7.07	7.06	7.06	7.06	6.00	5.71	5.39
Partial R <sup>2</sup>	.0100	.0097	.0097	.0097	.0081	.0080	.0083
<i>Added covariates</i>							
PDRs (95-99)		X	X	X	X	X	X
PDRs (99-03)			X	X	X	X	X
Δ income/wealth (95-99) <sup>a</sup>				X	X	X	X
county fixed effects, plus nbhd/munic unemp rate and mean income (1999) <sup>b</sup>					X	X	X
<i>Sub-samples</i>							
employed same plant 95-99						X	X
stable/growing plant 95-99 <sup>c</sup>							X
mean	.069	.069	.069	.069	.069	.071	.071
N	378148	378148	378148	378147	378147	229839	115015

*Notes:* 2SLS estimates using preferred instrument set. All estimates adjusted for peer group, neighborhood and municipality characteristics in 1995, and individual characteristics in 1999. Conventional robust standard errors in parentheses, corrected for non-independent residuals within neighborhoods. <sup>+</sup>, <sup>\*</sup> and <sup>\*\*</sup> denote significance at the 10, 5 and 1 percent level.

<sup>a</sup> Entered as third-order terms for change in personal income, change in other household income and change in household wealth. One observation missing household wealth in 1995 was omitted.

<sup>b</sup> Entered as second-order terms.

<sup>c</sup> 1999 plant of employment has at least as many FTEs in 1999 as in 1995.

**Table 5: Specification Tests: Employment Status and Sick Money Utilization by Year**

	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003
<b>Panel A</b>												
Dependent variable: Employed FT/PT at end of year												
Peer 2000 DP rate	.298* (.152)	.399** (.134)	.148 <sup>+</sup> (.087)	--	-.030 (.090)	-.161 <sup>+</sup> (.090)	.046 (.070)	--	-.360* (.149)	-.106 (.123)	-.125 (.147)	.010 (.154)
mean	.910	.928	.963	1.000	.978	.975	.980	1.000	.926	.937	.921	.906
<b>Panel B</b>												
Dependent variable: Received SM at end of year												
Peer 2000 DP rate	-.068 (.067)	-.228** (.072)	.017 (.068)	.048 (.075)	.097 (.078)	-.045 (.088)	-.033 (.084)	.201 <sup>+</sup> (.116)	.293* (.124)	.331** (.129)	.044 (.148)	.278* (.137)
mean	.022	.022	.022	.025	.031	.035	.037	.067	.068	.072	.075	.077
N	377135	377329	377733	378148	377859	377747	377839	378148	366250	354593	340962	326283

*Notes:* 2SLS estimates using preferred instrument set. All estimates adjusted for peer group, neighborhood and municipality characteristics in 1995, and individual characteristics in 1999. Conventional robust standard errors in parentheses, corrected for non-independent residuals within neighborhoods. <sup>+</sup>, \* and \*\* denote significance at the 10, 5 and 1 percent level. Results for years 2000-2003 omit workers who died or emigrated, and those drawing early retirement (AFP) or DP. Differences in sample size prior to 2000 reflects persons missing individuals records in *FD-trygd* in a given year.

**Table 6: Specification Tests: Neighborhood Disability Participation and Employment Rates by Year**

	1992	1993	1994	1995
<b>Panel A</b>				
Dependent variable: Neighborhood DP rate				
Peer 2000 DP rate	.061 (.086)	.029 (.089)	.031 (.090)	.047 (.091)
mean	.087	.094	.105	.116
<b>Panel B</b>				
Dependent variable: Neighborhood LFP rate				
Peer 2000 DP rate	.181 (.164)	.204 (.152)	.128 (.144)	.033 (.148)
mean	.622	.619	.625	.622

Notes: N=378148. 2SLS estimates using preferred instrument set. Dependent variables calculated over persons age 41-62 in 1995 neighborhood, defined as fraction receiving DP at end of year (Panel A), or fraction employed FT or PT at end of year (Panel B). All estimates adjusted for peer group, neighborhood and municipality characteristics in 1995, and individual characteristics in 1999, excluding neighborhood-level employment/program status variables (i.e. fraction employed FT, employed PT, employed MPT, self-employed, receiving permanent DP, receiving temporary DP, receiving rehabilitation pension, receiving day money, and unemployed without day money). Conventional robust standard errors in parentheses, corrected for non-independent residuals within neighborhoods. <sup>+</sup>, \* and \*\* denote significance at the 10, 5 and 1 percent level.

**Table 7: Effect of Social Interaction on Disability Pension Entry by Year**

Dependent variable: DP utilization in year				
	2000	2001	2002	2003
Peer 2000 DP rate	.137** (.050)	.180* (.074)	.313** (.097)	.504** (.123)
mean	.013	.029	.049	.069
N	378148	378148	378148	378148

Notes: N=378148. Notes: 2SLS estimates using preferred instrument set. All estimates adjusted for peer group, neighborhood and municipality characteristics in 1995, and individual characteristics in 1999. Conventional robust standard errors in parentheses, corrected for non-independent residuals within neighborhoods. <sup>+</sup>, \* and \*\* denote significance at the 10, 5 and 1 percent level.